The role of contaminants in the collapse of fish populations from the San Francisco Estuary and its watershed: complex mixture effects on embiotocids and salmonids

Kathrine R. Springman

Public Comments

No public comments were received for this proposal.

Collaboration Panel Review

Proposal Title

#0280: The role of contaminants in the collapse of fish populations from the San Francisco Estuary and its watershed: complex mixture effects on embiotocids and salmonids

Final Panel Rating
adequate

Collaboration Panel (Primary) Review

Collaboration:

Will the results of the collaborative effort be greater than the sum of its parts? Is it clear why the subprojects are part of a larger collaborative proposal rather than several independent smaller ones?

This $906K project teams up UCD and Applied Marine Sciences (AMS) in using an integrative field bioconcentration sampling method with a variety of lab assays to impute the impacts of contaminants on shiner surf perch and salmon. AMS receives about 25% of the budget so this is truly a collaborative proposal, but the project revolves around the PI who is fully funded for three years with a variety of other institutions (Bodega Marine Lab, DFG, Clear Springs Food) pulled into collaborate on specific pieces.

Interdependence And Integration:

Does the proposal have an example that clearly articulates the conceptual model of each subproject and how they link together as a whole? Are the boundaries of the study plans focused and cohesive, yet well delineated? Is there a plan for potential differences in the stages of subproject completion times? Are there clear plans for analyses and interpretations which seek to identify and quantify relationships among the data collected in various subprojects rather than separate analyses for each subproject?

The individual tasks fit together coherently. There's a good

rationale to the proposed project. . The end result is a nice integrative project, but the description of the final integrative model was vague enough that I wasn't certain it would work.

Project Management:

Is it clear who will be performing management tasks and administration of the project? Are there resources set aside for project management and time given for investigators to collaborate? Is there a process for making decisions during the course of the project? Are there acknowledgments of potential barriers to collaboration and explanations of how team members will overcome barriers particular to their institutions?

The PI has put together a very clear schedule of activities; however, the proposal indicates that it must start in two months, which seems very unlikely. The cascade of sampling is piggy-backed on other projects so it is not clear how the project will be rotated around a different start date. In addition, a lot of disparate data must arrive on time from a number of different labs. What happens if they slip? The proposal needs to discuss these possibilities and how to solve them. A web site is proposed as a way of tracking data and project issues.

Team Composition:

Does the lead principal investigator have successful management history and experience leading collaborative teams? Is it clear that all key personnel are committed to making significant contributions to the project? Do team members have complementary skills?

All the decision-making flows from the PI. Her co-PI has a strong publication record and she has experience working with the other team members so she should be able to pull it off.

Communication Of Results:

Is there a clear plan for comprehensive and cohesive reporting of project progress to the CALFED community?

Nothing proposed.

Additional Comments:

Collaboration Panel (Discussion) Review

Primary reviewer felt the proposal is Adequate. The details that weakened the proposal were: the final description of integrative model was vague; there was a clear schedule of activities, but start date is slated for June 2005 and does not address slippage in time; also, there is no mention of communication of results. Secondary reviewer vacillated between adequate and above average. Secondary evaluated team composition and project management lower. In retrospect, with addition of primary's concerns, decides it ranks as Adequate.

Technical Synthesis Panel Review

Proposal Title

#0280: The role of contaminants in the collapse of fish populations from the San Francisco Estuary and its watershed: complex mixture effects on embiotocids and salmonids

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The goals of this project are clearly stated and internally consistent, and the justification is well prepared. The approach is satisfactory but with some flaws, with field and lab components. There are some problems with monitoring and concentration techniques that may affect outcome and usefulness. Overall, the study is complex and some details on some of the methods/procedures are incomplete. While the use of SPMD devices to accumulate time-averaged samples is commendable, it is well known that these devices will not effectively concentrate all compounds of interest with equal efficiency, meaning that the concentrate will not accurately reflect the natural water composition. Removal of compounds from the SPMD and further workup also can yield artifacts, which will introduce uncertainty into the modeling. It is doubtful that the use of these extracts in exposure studies on collected fish will really simulate exposure/effects in natural systems. Moreover, it appears that the number of fish used in these studies is very small, which along with the absence of replicae experiments raises concerns about the validity of the exposure and effect studies. The model is poorly described and it is not clear how habitat and other confounding variables will be handled? Reviewers also had concerns that the monitoring with the SPMD devices may not be

sufficient in duration/period to satisfy critics. Three months of accumulation of the organics on SPMD during one summer period at eleven sites is proposed, under conditions that may be unusual at the chosen time. No followup verification is proposed. Will this yield much information beyond the years of monitoring data already available at these sites? Will their methods be consistent with those already used so that monitoring data can be compared with the existing data bases? Overall, the reviewers agree that the study could provide interesting information, but it is fraught with these uncertainties. It is not clear how useful the model will be in choosing management options. The reviewers also note that the PI has limited demonstrated capability to lead a team in such a multifaceted study, although the team appears to be well qualified and the budget is appropriate.

Additional Comments:

This project will almost certainly generate informatioon that will be of interest to the community. However, the value of this project will not be in the demonstration that potentially demaging organic compounds are present in the delta waters, that is already known, as pointed out in the proposal. Nor will it be surprising if the SPDM concentrate will have physiologic effects on surfperch or trout. Rather the team will be challenged to demonstrate a credible relationship between physiologic effects of the mixture of organic compounds at the levels they occur in the delta water and observed population declines. The lack of detail about the modeling approach makes it difficult for the reviewers to judge if the team is likely to be successful in this attempt.

The goals of this project are clearly stated and internally consistent, and the justification is well prepared. The approach is satisfactory but with some flaws, with field and lab components. There are some problems with monitoring and concentration techniques that may affect outcome and usefulness. Overall, the study is complex and some details on some of the methods/procedures are incomplete. While the use of SPMD devices to accumulate time-averaged samples is

commendable, it is well known that these devices will not effectively concentrate all compounds of interest with equal efficiency, meaning that the concentrate will not accurately reflect the natural water composition. Removal of compounds from the SPMD and further workup also can yield artifacts, which will introduce uncertainty into the modeling. It is doubtful that the use of these extracts in exposure studies on collected fish will really simulate exposure/effects in natural systems. Moreover, it appears that the number of fish used in these studies is very small, which along with the absence of replicae experiments raises concerns about the validity of the exposure and effect studies. The model is poorly described and it is not clear how habitat and other confounding variables will be handled? Reviewers also had concerns that the monitoring with the SPMD devices may not be sufficient in duration/period to satisfy critics. Three months of accumulation of the organics on SPMD during one summer period at eleven sites is proposed, under conditions that may be unusual at the chosen time. No followup verification is proposed. Will this yield much information beyond the years of monitoring data already available at these sites? Will their methods be consistent with those already used so that monitoring data can be compared with the existing data bases? Overall, the reviewers agree that the study could provide interesting information, but it is fraught with these uncertainties. It is not clear how useful the model will be in choosing management options. The reviewers also note that the PI has limited demonstrated capability to lead a team in such a multifaceted study, although the team appears to be well qualified and the budget is appropriate.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The panel believed that this research might produce results that are novel, interesting, and useful. The positive aspects of the proposal are that it deals with a live-bearing fish species that is of interest to CBDA. But, there are multiple, methodological challenges involved in the proposed research. Some of these challenges are not unique to the proposal, but

are indigenous to ecotoxicology today. Some revolve around the multiple steps required to determine the concentration and identity of contaminants in water that these fish utilize. The panel recognized that it is very difficult to extrapolate from a complex mixture of potential contaminants to exposure of fish to eventual uptake of these contaminants. The external reviewers recognized these and other methodological challenges and the panel agreed that it was possible, though not likely, that the applicants could overcome them.

Rating: Adequate

Technical Review #1

proposal title: The role of contaminants in the collapse of fish populations from the San Francisco Estuary and its watershed: complex mixture effects on embiotocids and salmonids

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goal of this 3 year study (in the proposal summary) was to investigate the "long-term multigenerational chronic effects of low level organic contaminants on fish populations in the San Francisco Estuary." by characterizing "bioavailable" organic contaminants in the SFE and relating these to biomarker and reproductive endpoints of two fish species. These data in turn were to be modeled using an approach called "Vitality Theory" to predict population effects on embiotocids and salmonids. Specific objectives were to look at several biomarkers in field-collected and lab exposed surfperch and in lab exposed salmonids. The working hypothesis states that bioavailable mixtures have altered the "fitness" (toxicity, immunosuppression, reduced growth and fecundity) of key fish species, resulting in effects (declines) in situ at the population level. No specific testable hypotheses were given however. This is a decades-long issue that remains timely and important in large part due to the interplay among changing natural and anthropogenic factors that influence the structure and function of aquatic ecosystems.
Rating	good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	The authors presented a conceptual model based on shiner surfperch (Cymatogaster aggregata), a live bearing indigenous SFE species, that links contaminant mixtures and several physiological processes (mostly reproduction related) to the reported population decline for this and other Bay species (Fig. 1). The selection of shiner is rationalized based on its uniqueness as a "live bearer" of offspring (and thus ease of measuring reproductive biomarkers) whereas the selection of rainbow trout for lab testing was based on a limited discussion of possible contaminant effects in salmonids. Little to no information on evidence of reproductive insult in SFE fish was given. The author(s) incorrectly assume that all contaminants are accumulated. The hypothesis (section 1.1.3) was very general and was thus not testable using standard statistical methods. No detailed mechanisms of contaminant insult or specific link to or evidence of contaminant effects in SFE populations of either species were presented, e.g. the wealth of literature on PAH effects (albeit at relatively high concentrations) on flatfishes in the Pacific Northwest. The model focuses on contaminants but is incomplete as it does not acknowledge other fundamental factors (climate, competition from other species). The use of semipermeable membrane devices (SPMDs) as biomimics to collect the bioavailable fraction of organics was a good, novel inclusion. However, no mention of existing or past data in the SFE was made. In general, the arguments made were less than compelling to suggest that contaminant effects are likely to be expected in this setting.
Rating	fair

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	A combination of field and lab work was proposed. First, SPMDs are deployed at 11 stations throughout the SFE to collect representative in situ bioavailable contaminants, an approach that could be improved by compositing accumulated contaminants from multiple deployments (e.g. during the wet and dry seasons) to ensure capture of a representative sample. The subsequent description of target analyte classes (mostly hydrophobic compounds) and analysis was extremely limited, e.g. no details were given on techniques, past performance or what compounds could even be expected from a hexane extraction of triolein. More importantly, the proposed contaminant list does not include many important and relevant contaminants of concern, including pharmaceuticals, current use pesticides or naturally occurring compounds (steroids, hormones). Field caught shiner will be analyzed for accumulated contaminants (even though many "toxic" contaminants may not be bioaccumulative) and a host of biomarkers including P450 induction (EROD, MFOs), reproduction related proteins, and multiple tissue histopathological examination. Lab maintained surfperch and trout will be injected with a "cleaned up" SPMD extract at three different locales -- Bodega Marine Lab, Pacific Biological Station (BC) and Clear Spring Foods (ID). Shiners collected from "uncontaminated" sites will be injected with the bioavailable extract from each of 11 RMP stations, ten fish per treatment. No mention was made of why females and males were to be used, how they were to be split among treatments, or the implications thereof. Little detail was provided on what baseline response in biomarkers were to be expected. "Viral

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challenge" experiments performed by the lead PI for her Ph.D. dissertation are to be performed on both species. It was unclear why three different locations are needed.

Modeling of data was described as a three step process: (1) determination of a site-specific fitness index, (2) correlation of fitness, SPMD results, and "habitat specific information" (?), and (3) application of "Vitality Theory", a model construct unfamiliar to this reviewer and that unfortunately was not adequately described. The authors acknowledge the potential fuzziness of this approach and offer a remedy based on multivariate analyses. Although my strength is not in ecological modeling, this section was exceedingly difficult to follow and/or understand.

Rating	fair

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	Fish collection and biomarker analyses were straightforward. It was difficult in general, however, to determine the feasibility of this project because of a lack of key details, such as which and how contaminant analyses will be performed, total number of water and/or fish samples for analysis, and an organized listing of parameters measured and analyzed for each experiment. Unless one was familiar in advance with the key modeling effort (Vitality Theory), it is difficult to comment on whether this model is applicable to the data to be collected.
Rating	fair

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	N/A
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	This section was extremely limited in scope. A website was described as the main tool for dissemination of project results, which as stated would only "be made available to project participants". No mention of data sharing (outside of project participants), publications, presentations or concrete data management strategies were made.
Rating	poor

Additional Comments

Comments	The proposal had numerous grammatical flaws, run-on sentences as well as questionable statements. As such, it read as if it was put together at the last minute, not carefully integrated nor proofread.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The lead PI is an entry level scientist at UC Davis approximately 5 years post Ph.D. Based on post-Ph.D. work experience (5 jobs in 5 years, none as an

	independent scientist and no external grants as PI listed), it is questionable whether the applicant has the experience and breadth of knowledge to manage this large (9 listed co-PIs representing 8 entities), interdisciplinary project. The remainder of the team is a mix of contractor, academic and private industry personnel, some of whom are recognizable as solid scientists in their respective fields.
Rating	fair

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The total requested budget ($906K) appeared to be reasonable for the proposed scope of work, although rationale for cost estimation was scarce. The majority of the budget was for lead PIs salary and expenses for co-PI Spies who heads up a consulting firm (AMS) with past/current involvement with the RMP.
Rating	good

Overall

Provide a brief explanation of your summary rating.

Comments	There were many flaws in this study, including non-specific/testable hypotheses, limited contaminant capture and potential relevance, and a lack of details for key analyses and modeling efforts. These flaws were exacerbated by the relatively low quality of writing and organization. For these reasons, I grade this proposal as FAIR.
Rating	fair

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Technical Review #2

proposal title: The role of contaminants in the collapse of fish populations from the San Francisco Estuary and its watershed: complex mixture effects on embiotocids and salmonids

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The basic questions asked in this study are fairly straightforward and are stated clearly. The goals are appropriate and certainly seem to be timely. Although I have some questions on the details, it is important that the authors intend to use a modeling approach to make the transition from individual to population level responses.
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	The need for the study is appropriately justified and the conceptual model is quite helpful. Clearly, in the push to restore Delta habitats, more effort has been placed on physical habitat concerns than on issues surrounding the impacts of contaminants on the recovery of fish population. The two study species, Cymatogaster aggregata and Oncorhynchus mykiss seem appropriate in that the former was historically a major component of the estuarine fish fauna and, as a species in the family Embiotocidae, is a livebearer (this life history mode simplifies some of the

	experimental design). Oncorhynchus mykiss is a common freshwater species in the region, is recreationally and commercially important, and is also widely used in laboratory studies so that procedures for its culture and maintenance are well established. The authors, however, incorrectly refer to embiotocids, including their study species, as ovoviviparous. Ovoviviparity refers to species with internal fertilization but in which all nutrient material is supplied to the developing young through the egg yolk. In fact, embiotocids are an excellent example of a viviparous species- those in which the developing young receive nutrients directly from the mother (e.g., Moyle and Cech, 2004, Fishes, an introduction to ichthyology, 5th edition). It is somewhat surprising that other key papers on California embioticids, including those dealing with feeding (perhaps important in pollutant uptake) are not cited as they certainly would pertain to the background of the study (e.g., Baltz, Donald M. 1984. Life History Variation Among Female Surfperches (Perciformes: Embiotocidae). Environmental Biology of Fishes 10:159-171; Ebeling, Alfred W., and David R. Laur. 1986. Foraging in Surfperches: Resource Partitioning or Individualistic Responses? Env. Biol. Fish. 16:123-133; Stouder, Deanna J. 1987. Effects of a Severe-Weather Disturbance on Foraging Patterns Within a California Surfperch Guild. J. Exp. Mar. Biol. Ecol. 114:73-84).
Rating	good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	In the broad sense, the study seems to be designed appropriately. I particularly like the collection of contaminants in-situ,

rather than testing the effects of specific compounds in isolation. However, I am concerned about the apparent lack of replication of the SPMD's at the 11 sites. No mention is made of how SPMD's will be located at each general site (i.e., randomly, fixed point, etc.) and whether more than one will be used per site (in the table showing tasks and subtasks, p. 48, it is mentioned that there are 5 SPMD's per cage). Normal procedures of experimental design would dictate that within-site variation needs to be established before comparisons can be made among sites. If there is not replication, this is perhaps due to the high cost of analysis (as suggested by the budget pages). However, the reasons for the lack of replication (if true) need to be stated. In the data analysis, each cage, with its 5 SPMD's, should be treated as a single replicate. There is some inconsistency with the fish sampling sites. Fig. 3 shows five sample locations whereas the text refers to only four. One of the stated objectives is to study the impact of organic contaminants on juvenile growth of shiner surfperch. In section 2.1.2, the authors indicate that the field study, "... will compare reproductive and growth measurements in field collected shiner surfperch....." However, nothing is said in this or any other section as to how growth will be assessed. Will this strictly be a comparison of juvenile mass/length relationships or will actual growth rates be determined through an analysis of suitable aging structures such as otoliths? An important aspect of the study is the comparison biological and chemical indicators of shiner surfperch from uncontaminated (i.e., control) versus contaminated (i.e., test) sites. Although possible control sites

	are mentioned, there is not any documentation given that would indicate that these sites are suitable controls. Given the importance to the study, the authors need to indicate what their basis is for making a decision about what sites are "relatively uncontaminated." Although I appreciate the importance of the modeling section, I have several problems with it (perhaps reflecting my lack of familiarity with the particular modeling approach proposed for this study). The model will apparently be parameterized in part by field data, including, "population levels and habitat data." However, no mention is made in the approach section about how trawl data (which are notoriously variable) are going to provide data on population levels. Furthermore, there is no mention of how or what habitat data might be collected. Without this information, it is difficult at best to evaluate how the modeling approach might succeed.
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	Completion of the main objectives, although ambitious, seems quite feasible. I do have questions about the potential for success of the modeling approach (see previous section). In addition, there is not sufficient information to evaluate how the impact of pollutants on growth will be determined (see also previous section).
Rating	good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?
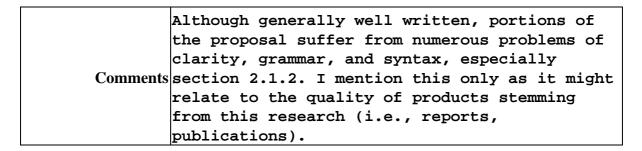
Comments	This is not a monitoring proposal
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The study has the potential to contribute very important information about the impact of chronic pollutants on native fish species. This information would seem to fit in well with other ongoing projects in the Delta region. The project should easily yield a number of meaningful publications in the peer-reviewed literature. On the negative side, there are some gaps in the study design that make it difficult to fully understand how some objectives (and thus products related to those objectives) will be met.
Rating	good

Additional Comments

Comments	Although generally well written, portions of the proposal suffer from numerous problems of clarity, grammar, and syntax, especially section 2.1.2. I mention this only as it might relate to the quality of products stemming from this research (i.e., reports, publications).

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Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	Overall the authors seem to be well qualified to complete this ambitious proposal and have the appropriate infrastructure. I am concerned that the senior principal investigator on the project (Springman) has only published one paper since receiving her Ph.D. in 2000. This might not bode well for timely publication of the results of the proposed study. However, others on the team have exceptional credentials, are world-renowned scientists (especially Emlen and Spies), and have very strong publication records.
Rating	very good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	This is an expensive proposal, however, it is also a complex, multidisciplinary proposal that has the potential to deliver some important information. Especially given the number of personnel (necessary for the complexity of the project) and the number of chemical assays, the proposed three-year budget is appropriate.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	

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	This is a complex, multi-disciplinary proposal that melds the expertise of six Ph.D.'s, one DVM, and three other scientists or managers. It addresses a real need in that the impact of chronic pollutants could hamper recovery of native fishes even if physical habitat issues are resolved. The proposal is generally well conceived and written, although there are some weak sections and there seem to be some important gaps in the experimental design.
Rating	very good

Technical Review #3

proposal title: The role of contaminants in the collapse of fish populations from the San Francisco Estuary and its watershed: complex mixture effects on embiotocids and salmonids

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goals and objectives of the project are clearly stated (though no detailed hypotheses are given). The idea is timely and important; there are insufficient data available for assessing the influence of long-term exposure of resident fish in the estuary to complex mixtures of contaminants and how these effects may contribute to the observed declines of the surfperch populations.
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	The study is justified relative to existing knowledge; it is indeed unclear if exposure to complex mixtures of contaminants could have contributed to the decline of surfperch populations and may be hampering recovery after habitat reconstruction. The conceptual model explains the underlying basis for the proposed work; the proposed work is a logical expansion of the project objectives. One minor problem is that the objectives section intended to refer to a section that apparently listed the bioavailable contaminant

	mixtures in the San Francisco Estuary, but this does not seem to have made the final version of the proposal.
Rating	very good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The general approach for meeting the project's objectives are well designed. The shiner surfperch is a very good choice as the main subject of this study, as it is a member of a group of fish that has been declining, is known to accumulate high concentrations of organic contaminants, and is a live-bearer in which females carry their entire annual reproductive output (making it easy to study effects on reproduction and on developing young). The project contains a nice combination of field and laboratory approaches. In the field, organic contaminant mixtures will be collected (for analysis and for use in experiments) and fish will be collected (and analyzed) for correlative work on contaminant effects. In the lab, fish will be exposed to contaminants collected at the field sites with the use of semi-permeable membrane devices (SPMDs), and will be immunochallenged it combination with the contaminant exposures. The use of SPMDs offers a nice way to obtain time-integrated samples for quantifying contamination levels at the sites and to obtain the actual mixtures as they exist in the field for use in the laboratory approaches. One weakness is that the dosing method (intraperitoneal injection of the SPMD extracts) is artificial; actual dosing in food or water would be more realistic. However, it would require larger amounts (of contaminants) than are easily collected by SPMDs. Other options would be to first analyze the

concentrations in the SPMDs and to base (laboratory-made) mixtures for exposures on those results, or to use field-collected water and sediments. While the general approach is clear, details are missing in the workplan for many parts of the project. E.g. will there be multiple SPMDs placed at each of the 11 field sites, or just 1 per site? Will shiner surfperch be collected at all 11 field sites, or at just 4 sites ("Our goal is to sample two sites that are contaminated and two that are least contaminated")? How many fish will be collected at each site? The only reference to sample size is that "in some pregnant females (10) whole body chemical analyses of the mother and young will be carried out to provide a picture of transplacental transport of contaminants". Will this be done for each of the sites? The budget for task 6c "chemical analysis of tissues of field-caught adults, juveniles and embryos" list a total of 25 samples. So apparently these analyses will be done for only 1 site? What criteria will be used for selecting this site? The lab experiment will be conducted with 10 fish per site, but this is divided over two sexes (and possibly different age groups – proposal makes special mention of pregnant males and juvenile males), while some exposed fish will be sacrificed on day 3 while others will be injected monthly for a 4-month period. This reduces the sample size to maybe 2 fish per group – too low for any meaningful statistical analyses. No sample size is mentioned for the shiner surfperch viral challenge experiment. The only experiment with a substantial sample size is the 2nd viral challenge experiment with rainbow trout; this experiment will be conducted with 3 replicates using 15 fish each. The modeling and data analyses for the project are only described in very general terms (such as "a variety of multivariate approaches will be used") and thus difficult to evaluate. It will be very difficult to relate contaminant levels for a long list of contaminants (PCBs, PAHs, PBDEs, etc.) to physiological responses. The goal appears to also

	include synergistic effects, making the list of variables a lot longer. The modeling also appears to include population dynamics variables. While the proposed sampling scheme will yield information on the reproductive rate (# embryos carried by the pregnant females), it is unclear how any data on the other population-level variables would be obtained.
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The approach is incompletely documented (see my comments under "Approach"), making it difficult to assess the likelihood of success. Certain aspects of the approach (such as sample size) appear insufficient in some cases. The scale of the project appears generally consistent with the objectives, but the lack of methodological details indicates that the authors have not carefully considered all the details needed to successfully complete the project.
Rating	good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	(not appropriate)
Rating	very good

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Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The project could potentially provide valuable contributions to efforts aimed at the recovery of surfperch in the San Francisco Bay area.
Rating	very good

Additional Comments

Comments	The authors of the proposal should have expanded on the methodology (especially) and have described the infrastructure available for conducting the research. The main part of the proposal is nowhere near its page limit, leaving plenty of space to address methodologies in more detail.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	While most of the collaborators have a strong track record of past performance, the PI's is very small (no grants, one publication in the area of this proposal). The PIs dissertation was on absorbents for contaminant removal from groundwater. The infrastructure for accomplishing the project's goals are not elaborated in the proposal. Based on past performance, the collaborators do have the infrastructure for completing their tasks.
Rating	good

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Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget seems generally reasonably for the proposed work. In a few instances the time commitments seem excessive, such as the PI's 6 month salary for the modeling work (which seems to be mostly conducted by John Emlen anyway), the PI's 3 months for morphological analyses of the surfperch, and the 50k for manuscript preparation by one subcontractor.
Rating	good

Overall

Provide a brief explanation of your summary rating.

Comments	In principle, the proposed project is an elegant approach for evaluating an issue that is important to the region. However, sample size is in some instances insufficient and in many instances there is a lack of details needed for evaluating the likelihood of success. Since the PI has no proven record in the topic of the project and not yet had a chance to develop a track record of successfully managing large research projects, I feel that we can not just assume that PI will easily overcome the (albeit not overly major) inadequacies and the lack of consideration of the project's details.
Rating	good